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CONTENTS

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THE PSYCHOLOGICAL BULLETIN

FURTHER STATISTICS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

BY SAMUEL W. FERNBERGER

University of Pennsylvania

Boring[1] has shown that the center of population of psychologists lies somewhere in central Pennsylvania. Another statistical treatment of the data of the membership of the American Psychological Association, namely, where psychologists did their major graduate work as indicated by the place of the taking of the Ph.D. degree, is also of interest in this connection. Data were obtained from the 1921 Year Book of the Association. A total of 377 members report the Ph.D. degree and the institution from which it was obtained. Of these, 36 are foreign degrees as follows: 14, Leipzig; 4 each from Berlin and Würzburg; 3 each from Freiburg and Zurick; 2 each from Bonn and Halle; 1 each from Glasgow, Heidelberg, Jena and Strassburg. Thus the total number of American Ph.D.'s who also indicate the place of study is reduced to 341 which are distributed as follows: 67, Columbia; 54, Chicago; 43, Harvard; 41, Clark; 33, Cornell; 19, Yale; 17, Hopkins; 16, Pennsylvania; 7, Iowa; 6, Michigan; 5, Stanford; 4, Illinois; 3 each from Geo. Washington, Princeton and Radcliffe; 2 each from Boston, Brown, Byrn Mawr, Catholic, and Wisconsin; 1 each from Amherst, California, Illinois Wesleyan, Indiana, Minnesota, Missouri, Nebraska, New York University, Ohio State, and Union.

Hence it appears that 75.1 per cent. of the American doctorates were obtained east of the Appalachian Mountains and 94.7 per cent. were obtained east of the Mississippi. Twice as many psychologists in America received their degrees abroad as received

[1] BORING, E. G., PSYCHOL. BULL., 1920, 17, 271–278; 1921, 18, 64.

them west of the Mississippi. Professional American psychology seems to be largely an eastern product.

One even more astonishing fact to be derived from these data is the relative dominance of a few institutions. In all, 30 institutions have granted a Ph.D. to at least one member of the American Psychological Association. Of these, five (Columbia, Chicago, Harvard, Clark and Cornell) have together granted 238 or 69.8 per cent. of all of the doctorates. Eight institutions (the five just mentioned plus Yale, Hopkins and Pennsylvania) have granted 290 or 85.0 per cent. of all of the American doctorates. The remaining 22 institutions offering work for the Ph.D. degree have only granted 51 doctorates or 15 per cent. of the total number. It would seem that relatively few institutions in this country are producing the majority of men qualified for academic positions and hence are exerting an extreme influence on psychological thought in America.

The situation with regard to where these men received their first psychological inspiration is very different, however. We may believe that a student receives his first inspiration at his undergraduate college or university and there acquires enough interest and curiosity with regard to the science to form the basis for his future psychological studies. We have therefore determined the frequency of individuals who have graduated from each undergraduate college represented in the Association Year Book. These data follow:

16, Indiana; 13, Chicago; 11 each from Cornell and Nebraska; 10 each from Harvard and Yale; 9 each from Amherst, California, and Michigan; 8 each from Columbia and Illinois; 7 each from Dartmouth, Pennsylvania, and Princeton; 6 each from Iowa, Stanford, and Vassar; 5 each from Missouri, Ohio State, Wellesley, Williams, and Wisconsin; 4 each from Brown, Oregon, and Wesleyan; 3 each from Bethany, Clark, De Pauw, Haverford, Holyoke, Kansas, Northwestern, Ohio Wesleyan, Texas, and Vermont; 2 each from Arkansas, Barnard, Beloit, Bucknell, Campbell, Cincinnati, Colorado, Cornell College, Earlham, Hiram, Hopkins, Lafayette, Leander Clark, Massachusetts Institute of Technology, Minnesota, Missouri Valley, Oberlin, Oklahoma, Randolph-Macon, Richmond, Rochester, Teachers College, Ursinus, Vanderbilt, Washington, and Wyoming; 1 each from Allegheny, Antioch, Augustana, Bates, Bowdoin, Bryn Mawr, Catholic, Central, Coe, College of the City of

New York, Cumberland, Davidson, Denver, Dennison, Drake, Emory, Evansville, Franklin and Marshall, Furman, Gettysburg, Grand Island, Greenville, Grinnell, Gustavus Adolphus, Hamilton, Hedding, Hobart, Illinois College, Illinois Wesleyan, Juniata, Legrange, Lebanon, Lombard, Michigan Agricultural College, Marietta, Monmouth, Muhlenberg, Muskingum, Nebraska State School, New Hampshire, Occidental, Olivet, Ouachita, Parsons, Radcliffe, Southwestern Presbyterian, Smith, South Carolina, South Dakota, Springfield Y. M. C. A., St. Andrews, St. Bonaventure, St. Lawrence, Swarthmore, Taylor, Temple, Texas Christian, Trinity (Conn.), Trinity (N. C.), Tufts, Tulane, Union, Valparaiso, Virginia, Wabash, West Virginia, West Virginia Wesleyan, William and Mary, and Wilson.

Besides these, 16 persons received their inspiration outside of the United States as follows: Toronto, 5; McGill, 3; Oxford, 2; and one each from Acadia, Cambridge, Dalhousie, Edinburgh, Glasgow and Neuchatel.

In the American "inspiration" list we find 105 institutions not represented in the list of institutions granting the Ph.D. Also, curiously enough, we find three institutions in the Ph.D. list which are not represented among the undergraduate colleges (Boston, George Washington, New York).

Another fact that becomes evident from a study of the two lists is the loss of supremacy of the small number of universities granting the Ph.D.'s degree. While the five institutions (Columbia, Chicago, Harvard, Clark and Cornell) granted 69.8 per cent. of the American Ph.Ds., they only inspired 13.4 per cent. for psychological work. And while the eight institutions (the five just mentioned plus Yale, Hopkins and Pennsylvania) granted 85.0 per cent., they only inspired 19.0 per cent. for psychological work.

The east also loses its supremacy in the matter of inspiration. One finds that only 48.2 per cent. received their initial inspiration east of the Appalachian Mountains while 75.1 per cent. were granted Ph.D. degrees in the same locality. Also only 76.4 per cent. received their inspiration east of the Mississippi as against 94.7 per cent. of doctorates from the same region. The geographical location of six institutions could not be determined in the *World Almanac* but their inclusion would make very little difference in the percentages.

Finally only 17.4 per cent. completed their graduate work in

the same institution at which they received their initial inspiration. These facts would seem to indicate that, although the interest in psychology is widespread in this country and although one finds psychologists everywhere who are capable of arousing a lasting interest in the work among their students, still the number of laboratories, equipped to give adequate postgraduate work leading to the Ph.D. degree, is very small.

HABIT FORMATION IN ANIMALS

BY OAKLAND MAUPIN

University of Kansas

INTRODUCTION

The general purpose of this paper is to review the experimental work published during the last decade in the field of comparative psychology on the general topic of habit formation. No reference has been made to tests of discrimination, studies of instincts, or studies of the higher mental processes, unless they have particular value in explaining or amplifying the conclusions drawn from the experimental work in the field of habit formation. The principal object of the paper is to show the present status of the various problems which have been attacked, and to point out, where possible, the next lines of approach, rather than to review the procedure of individual experiments. Such reports may be found in the yearly reviews of the BULLETIN. To the present writer they have been of great service in the compiling of a bibliography for this paper.

The present decade has seen the establishment of animal psychology as legitimate and good psychology, and whereas in the last decade many papers were written defending the right of psychologists to study the problems included in this paper, there is no longer any need for defense, and it has consequently disappeared from the literature. That such discussion has ended is a tribute to the work of the experimenters in this field. Their researches have not only resulted in the improvement of the methods employed in their own problems, but have lent a new tone to the entire field of psychology. This is shown in the more objective controls used in experimental work and the tendency to state results in behavioristic terms. Development has been very rapid, for scarcely more than twenty years ago, most of the publications in animal behavior were little more than anecdotes relating the accomplishments of pets. Even when definite experimental work was begun, the general method consisted in the study of a few animals in many situations, and long monographs were published giving descrip-

tions of the reaction of these animals in the various problems. Though this practice has extended somewhat into the present decade, the custom is to use a large number of animals, and to limit the problem as far as possible, in order that adequate controls may be made and reliable results obtained.

Notwithstanding the great improvement in experimental method, few real contributions have resulted, because the inclination during the present decade has been to attack a large number of problems, but to follow few to their final solution. As one reads paper after paper the same general conclusion can be found on the last page, viz., that the problem under consideration has not been solved and further experimentation is necessary to complete this study. Most of the larger problems have been worked upon, in order to fulfill a partial requirement for a doctor's degree. And, after enough has been done to warrant the degree, the experimenter has in most cases dropped the problem completely. Even though another experimenter studied the same general problem, he changed the method and procedure to such an extent that he did not further the previous investigation, but virtually started a new problem.

Because of the number and variety of the problems which are included here, it has been difficult to group them so as to treat them topically. For the purposes of discussion such an attempt has been made, which has necessitated that some papers be classified rather arbitrarily. The general topics which have occupied experimenters during the present decade are maze and problem box studies with special reference to: (1) the effect of certain factors (as age, sex, etc.) upon learning ability; (2) the discovery of an adequate criterion of learning in maze experiments; (3) the determination of the relative value of different curves of learning; (4) the use of the maze to test different species of animals; (5) study of retention of motor habits; (6) the sensory field most essential in maze learning; (7) theories of learning; (8) results of distribution of effort; (9) effects due to transfer of training. Problems 1, 2, 3, 4, and 6 occupied experimenters during the earlier part of the decade, and 5, 7, 8, and 9 during the latter part of this period.

For the solution of problems in the field of habit formation, no other apparatus ranks with the maze and no other animal with the white rat. With one or two exceptions this combination has been used for the solution of every major problem reported here. That this has been true seems due to a sort of inertia rather than to any

real justification. Dashiell (1920) discusses the need for an analytical study of the maze problem in which he says, "The maze method of studying the process of learning, as it is so commonly used in animal psychology and to some extent in human, is still employed almost entirely in an unanalyzed manner so far as concerns the apparatus. The particular pattern of labyrinth to be used by an experimenter seems to have been principally determined by the patterns used by earlier workers. The Hampton Court maze is the classic instance. A variation from earlier forms may be forced by such considerations as: the limitations of the learning capacity of the animal to be used; the limitations of available space; the nature of the problem, *e.g.*, whether motor or sensory habits; the limitations of the materials to be used in the construction, etc. Watson and his students have introduced the use of a circular maze, but its particular advantages have not been definitely set forth, so far as the writer is aware. A thoroughgoing and detailed planning of what may be called the architecture of the mazes that are to be used in a given research is at present impossible on account of our ignorance of the relative values of different elements within a maze problem."

A similar criticism might be made with reference to the white rat. Though it is generally conceded that the white rat because of his mode of living, well-known ability at motor learning, and small size is a convenient experimental animal, it is somewhat doubtful whether in comparative studies, it is justifiable to use any *one* animal to the exclusion of others. However, this is not the real problem. In any experiment, we have a situation which is more than an apparatus; it is an animal in an apparatus. Then, what we desire to know is whether consistent results can be obtained from this situation. Whether the Hampton Court maze or the Watson maze is superior or whether or not one maze pattern has advantages over another can not be determined by examining its parts logically. An apparatus might have perfect internal consistency and an animal might react normally in every other way, but when the two are combined as a basis for data from which one desires to draw general conclusions, so many chance factors may have entered that the situation has become unreliable for this purpose. Experimentation, covering the past three years (as yet unpublished in full), in the psychological laboratory at the University of Kansas has been concerned with the reliability of apparatus

and such results as have been obtained are sufficient to show that this is a problem worthy of notice. The solution of this problem will determine the value of a number of conclusions reached in the following experiments.

Effect of Certain Factors (Age, Sex, Etc.) on Learning Ability

Assuming, however, that the maze, or in a few cases a problem box, does test learning ability, a number of experimenters have attempted to use this apparatus for the solution of certain problems and to formulate general laws concerning learning. Hubbert (1915) states her problem as the relation of the age of an animal to its learning capacity. The white rat was the animal, and the test of its ability to learn was the Watson circular maze. Her problem was certainly practical in view of the fact that the white rat is the chosen experimental animal, and it is worth while to know when it reaches its period of greatest efficiency. 27 twenty-five day old rats, 27 sixty-five day old rats, 28 two hundred day old rats, 28 three hundred day old rats, and 12 five hundred day old rats were tested for their ability to learn the Watson circular maze. The number of trials, the total distance and the total time required for each rat to learn the maze were recorded, and the averages were computed for each age group. She found that "Young rats learn the maze more rapidly than the old ones, the rapidity with which the habit may be formed decreasing with increase in age," and in general, except in the very young and very old rats, the males were superior to the females, both in time required for learning, and in the absolute time required for running, when the habit was perfect. Disregarding her results which have been criticized by Paterson in a paper which will be reviewed below, the fact still remains, as noted above, that she was not justified in assuming that the maze was a reliable test of learning. If the test is a constant and she could show a difference in learning at different ages, then her conclusions would be valid, but if the test is a variable as well as the factors which may enter into the solution of it, the only results which are reliable are those that have to do with a particular instance. In any case, she is not justified in drawing this final conclusion which is the culmination of her paper: "If an analogy may be drawn between the learning ability of the rat and that of the human subject, it may be seen that in general the old can learn

a given problem as well as the young, although more effort is required to do so." It is impossible to determine what relation a sixty day old rat bears to a man, and besides in a problem of this nature, there is no reason why one should not work with human beings, if results which have reference to them are desired. That will always be the best way to solve human problems. The contributions of animal psychology to human psychology, at least as far as learning is concerned, have been the development of a behavioristic point of view and a method of approach to certain problems for which human subjects are hard to secure. Three other problems (discussed below), which have fallen into the same error as regards the reliability of the maze as a test of learning ability, are concerned with problems of this nature.

Basset (1914) performed an experiment to test the ability of a strain of albino rats of less than normal brain weight to form habits. Sixty-two rats of decreased brain weight were obtained by breeding out a branch of inferior rats. They were tested in the Watson circular maze and the inclined plane problem box, and were found to have less than normal ability to form habits, as measured by the ability of a control group of normal rats. Vincent (1915), in reviewing this paper, quotes Basset where he says, "It would seem (although lessening brain weight has ceased after the fourth generation of inbreds) that the ability to form habits lessened progressively with successive generations of inbreeding," and she adds the criticism "that the writer nowhere urges that the lesser ability was due to the inbreeding *per se*." If Basset secured rats, during the inbreeding, which were not inferior in brain weight to their species, this factor could easily have been controlled; if he did not, it would, of course, be impossible to determine what the *essential* cause of the decreased ability might have been.

This opinion is supported by an experiment of Ada Yerkes (1916), although the evidence is not conclusive, because of the small number of rats used in her experiment. She tested seven inbred rats of the thirteenth generation on two modified Watson mazes, one having a somewhat longer pathway than the other, and compared their records with those of nine stock rats. She found that the male stock rats in the long maze traveled greater distances, took a longer time for most of the trials, and made on the whole more errors, but acquired a perfect habit sooner than the inbred males. The inbred females, however, acquired a perfect habit

sooner than the stock females. On the long maze the stock rats on the average acquired the habit in 55.25 trials, as opposed to 65 trials for the inbreds; and the short maze in 19.2 trials, as opposed to 23.75 trials. There was greater irregularity in both the distance and error records for the inbred rats. Anatomically, these inbred rats were found to have a greater body weight and body length than the stock rats, but unlike Basset's rats the brain weight of the inbred rats was greater, not less, so that the relations of brain weight to body length and weight were nearly equal for stock and inbred rats, and very close to Basset's averages for normal animals. This would indicate that the inbreeding alone may have some effect on the maze learning.

Paterson (1917) questioned the treatment of the data in both Hubbert's and Basset's work in a paper which I consider particularly important, because it shows very clearly the value of treating data statistically. In discussing Hubbert's data he says, "It is concluded (on the basis of a comparison of averages) that the young rats (judged by the various criteria) learn more readily than the older rats. Because of the great differences between the rats in each age group it is doubtful if the conclusion is valid." He showed by calculating the average deviations of the number of trials required to learn the maze, that for the 25 day old rats, there was an average deviation of 7.3, the average being 30.4; for the 65 day old rats an average deviation of 9.3 with the average 30.7; and for the 200 day old rats an average deviation of 21.4 with the average at 41.8. By leaving out the records of six erratic rats of this 200 day old group, however, the average deviation is only 9.7, while the average drops to 32.6. He concluded, therefore, that no significant age differences were shown. I repeated Paterson's calculations for the distance criterion and obtained similar results. With regard to Basset's results Paterson says, "It is found, in roughly working out the medians and quartiles of the distribution according to the number of days required to learn and relearn, that there is a difference in favor of the control rats of only five days while the probable error for each median is rather large (8.5 for the controls and 11 for the inbreds). Further, the distributions for the two groups are practically the same, except that four inbred and one of the control rats are exceptionally poor. Did these four inbreds and the one control rat also have exceptionally smaller brain weights? This question can not be answered by reference to the data."

Other problems of similar nature are those of Arlitt (1919) on "The Effect of Alcohol on the Intelligent Behavior of the White Rat and its Progeny" and of Lashley (1917) on "The Effects of Strychnine and Caffeine upon the Rate of Learning." Arlitt found that the rats which were given doses of alcohol showed decreased learning capacity according to all three criteria, viz., trials, total time and total errors, in twenty-nine out of thirty cases, when their records were compared with the control group of normal rats. She, also, concluded "that parental alcoholism resulted in the lessened speed of running and in rate of learning, when the dose administered the parent animals has been small and the feeding period short." The defects in the descendants of these rats due to parental alcoholism are present in the second generation, but tend to breed out by the fourth generation. Because of the proportionately large number of deaths and the sterility of the alcoholics, the number in the case of the heredity tests was undoubtedly too small for any definite conclusions. With respect to caffeine and strychnine, Lashley found that caffeine caused an acceleration of learning, if given in large enough doses, while caffeine regularly interfered with learning; but, after the habit had been perfected, strychnine increased and caffeine decreased the accuracy of the habit. These two experiments, if they demonstrate nothing else, illustrate one of the advantages of animal psychology named above, that is, that experiments difficult to perform with human subjects may be performed upon the lower animals, and even if they are not directly analogous, at least, they show whether the same experiments in human psychology would be likely to be profitable, and what lines of observation to pursue.

The above experimenters have in most cases observed the difference in the ability of the males and females to learn a given problem, or rather the lack of difference, for in most cases the individual variations in each sex were far greater than the dissimilarity between the sexes. Sex difference has never been made the subject of a specific study in animal behavior, although it presents a most interesting problem.

An Adequate Criterion of Learning in Maze Experiments

Some discussion can be found in the literature as to what should constitute an adequate criterion of learning in maze experiments. It is generally conceded that learning is not complete until an error-

less trial has been run, and most experimenters have demanded that several successive perfect runs be made, so that the possibility of a perfect trial, due to chance alone, is eliminated. The number of such trials required has varied from 10 to 3, the present custom being to require 3 successive perfect runs.

Lashley, using the data from the experiment on the effect of drugs (discussed above), attempted to find out whether reliable results could be obtained, when one perfect trial had been run and without the prolonged training. In a paper called "The Criterion of Learning in Experiments with the Maze," he gives a correlation coefficient of $.632 \pm 0.061$ between the standing of the animals when tested by the criterion of one perfect trial and by the more difficult criterion of three perfect successive trials. He concluded that "There is no advantage, for the reliability of results, in prolonged training where the problem is that of a statistical comparison of different groups of animals by a single standard of achievement." Lashley's results, as he himself states, are not strictly comparable because of the different drugs administered to different groups, differences in the ages of the rats, the possible effect of seasonal differences, etc. These factors would, however, appear to lower the correlation. If the maze is to be used in psychological experimentation, it is important to know at what time in the maze learning sufficiently reliable results may be obtained for the relating of groups of animals. While in experiments like Lashley's, in which the effect of drugs is tested, it might not be valid to stop before the first perfect trial, because the effects of the drugs might not have appeared in the earlier runs, it is possible that the final standing of normal rats is determined long before the first perfect trial.

Relative Value of Different Curves of Learning

Where the maze has been used as a test of learning, still another problem has presented itself for solution, viz., what shall constitute learning the maze. Shall learning be judged by ability to run the maze quickly as measured by time units, or shall the errorless run, and consequently the shortest distance run, determine an animal's learning capacity? Yerkes, Carr, Watson, Ada Yerkes, Brockbank and Vincent have discussed this question incidental to their experimental work and Hicks and Hubbert have made it the subject of specific investigation. With these experimenters and

with others, there has been considerable disagreement both of opinion and practice in regard to the relative validity of the different criteria.

Yerkes (1907) in his *Dancing Mouse*, when reporting experiments done with the maze, says, "Time records are not recorded, because they are valueless as measures of rapidity of habit formation, for at any point in its progress the rat may stop to wash its face or to look around and a shock may hurry it into an error; a long trip as measured in time units does not necessarily indicate the lack of ability to follow the labyrinth path rapidly and accurately. *So the number of errors should be given first importance and time the second place.*"

Watson (1914), on the other hand, in his discussion of motor habits in general, says, "*time is the best single criterion of motor habits*," and "distance traversed, where it can be measured accurately, is probably the next best criterion." He states later, however, that the "mastery of the problem regardless of the time can not be said to have been attained until there is no excess distance," which would seem to imply that for Watson also the elimination of excess distance is the final test of learning.

Hicks (1911) who made this problem the subject of a specific investigation says in her discussion of the values of various curves of learning, "*The distance curve* portrays all of the eliminative process, and it *approximates the ideal of uniformity and regularity of descent.*" She considers, however, that such a criterion is impractical, because the rat runs so rapidly that it is impossible for one person to record and observe at the same time, and the work of transcribing a record into distance terms is very laborious. Therefore, she concludes, finally, that "Time is the best single criterion for adequate representation of the learning process, and the value of any curve depends upon the circumstances and no dogmatic statement can be made to cover all circumstances." This paper was of course written before the time of the Watson circular maze with its camera lucida attachment, which makes the record of total distance comparatively easy to secure. Whether it is as important as Mrs. Hicks believes, is yet to be determined.

Some work was done by Hubbert in 1914 and published under the title "Time versus Distance in Learning." The time used was the total time consumed from the point of entrance to the food box and the total distance covered in that time. Twenty-

seven rats were given two trials a day and the problem was considered learned after six consecutive perfect runs. The time and distance curves show a general similarity of contour. The "discrepancy" in the curve from the eighty-first to the eighty-sixth trials was explained by the author, as due to the erratic behavior of one rat which refused to run. Static time was not omitted. She states that it is impossible to determine from this experiment whether time or distance is the better criterion of learning, and concludes that "it seems wise to await a more complete study of the question before deciding."

Ada Yerkes in her study of inbred rats (discussed above) found that the time and distance curves were not similar. She says, "If either time or distance were taken alone, a true picture of the learning process would not be attained. The records of distance alone would make the inbred rats appear the quicker at first, in learning the correct path which the time records show was not the case. *A combination of the time and error or time and distance curves apparently gives the most adequate representation of the facts obtained* from these stock and inbred rats."

Vincent (1915) also noted that in her mazes with sensory stimuli (discussed below), the curves based on errors and time, respectively, were dissimilar. In the black-white and olfactory maze (trail in the true path) the errors were much reduced, but the time was very slow due to the dominance of the sensory stimuli. The rats in the olfactory maze, for example, kept their noses close to the beef extract path and ran slowly. These observations go to show that any distraction might retard the speed of the rat without affecting his accuracy.

Brockbank (1919) in a recent study of retention discards both the time and distance criteria, because he considers that they point out only minor factors of the essentials of learning and retention. He says, "The learning of the maze consists in the acquirement by the rat of certain integrated movements; and the *trials* after a period of disuse of these movements shows the loss in integrations of movements, if any. Time and distance as primary factors can not be employed except under almost impossible conditions to show what actually takes place in the learning of the maze, but they are valuable as secondary factors." He does not define impossible conditions and it would appear that the perfection of integration is as well shown by distance as by number of trials.

As far as practice is concerned, trials have been most generally used in measuring the total amount of effort required to learn the problem. Carr (1912) criticized this usage in his discussion of the relation of learning curves to learning capacity, because "the various trials are so radically disparate as to the time and effort expended that the assumption is without any rational basis." From a logical standpoint alone, this seems a valid objection, for during the progress of the first trials, some rats proceed almost to the center of the maze and then return to the entrance, only to repeat this procedure several times in succession, while another rat will, upon his approach to the center, accidently stumble into the food box. A certain number of trials, therefore, may represent a great variation in time and distance. For example, with Hubbert's rats the minimum number of trials for each group was 14; but the 25 day rat ran a distance of 30,348.8 cm., the 65 day rat ran 9,184 cm., and the 200 day rat ran 12,569.6 cm., although they each ran 14 trials. As shown with Hubbert's data above, then, a certain number of trials may represent a great variation in time and distance. However, though experimenters usually record time records and often the distance record, also, they rarely make use of them in discussions of their data but rather consider the average number of trials per group, instead of the total distance or total time. When using trials as a measure, Hubbert found that the 65 day rats were slightly inferior on the average to the 25 day rats, but as measured in distance units the average distance of the latter group was somewhat larger than that of the former. It may be that there is such a high correlation between the standings of individual animals or groups of animals, in total number of trials, total time, and total distance that any one taken alone is an adequate measure, but it is doubtful whether the number of trials required to learn the maze comes so close to measuring the actual effort expended as either of the other two criteria.

Previous to the use of the camera lucida attachment there was some discussion as to what should constitute a unit of error. All errors could scarcely be considered equal because in progressive elimination there are all degrees of error from thorough investigation of the blind alleys to mere hesitation at the openings of the cul-de-sacs. Because of the difficulty of evaluating these errors, the error criterion was almost worthless, and time certainly gave the better comparative measure. However, distance as it is now

recorded by using the camera lucida attachment with the Watson circular maze, includes the error curve. When the distance required for a perfect run is subtracted from an animal's distance record, there is left the total distance run in retracing the true path and in entering into and returning from blind alleys. In this way the error unit can be reduced to a half or quarter inch or to whatever such unit that is small enough to include all the partial errors.

Since the different curves of learning have been shown to be quite similar, it will probably not be possible to determine which criteria of learning is most satisfactory by studying the form of the curves. The problem might be approached by finding the relation between the various criteria by the method of correlation. If the correlation is high between two criteria, one may be used instead of the other in recording experimental data. If the maze is to be used in experimental work, this problem ought to be solved so that, if it is not necessary to take all three records, the experimenter may save time; and a measure can be established which can be used in a constant way in different laboratories. The need for a uniform measure is made more emphatic when the various maze experiments which have been done on different animals are brought together. For the convenience of the reader a brief outline of the salient points of these papers is given in tabular form below:

Referring to the above table, it may be noted that certain facts are established with reference to animals when particular mazes, criteria, method, etc., are used. No one could deny, however, that these results would have been much more valuable, if it were possible to compare the standings of the different animals with reference to a constant apparatus and method. The maze is a unique problem, and it would be of interest to know which animals are most proficient in this apparatus. Though it, perhaps, would not be possible to rank animals in an intelligence scale on the basis of the maze, or upon any other one apparatus, such information would help to establish this series. Intelligence is a difficult term to define with reference to an animal which can not be tested in a practical situation, and such a scale as we may be able to make will probably be dependent upon the number and complexity of the habits an animal can form. If a comparison is to be made using the maze apparatus, then a standardized maze should be

EXPERIMENTS USING THE MAZE WITH DIFFERENT SPECIES OF ANIMALS

Experimenter	Animal	Maze	Record	Trials per day	Criterion of Learning	Stimulus	Results	Retention
Thorndike (1911).....	6 chicks	Simple rectangular maze	Time trials	....	Perfect habit	Food, society	Habit was formed	Nearly perfect after 20 days
Kinnaman (1902).....	2 Macacus rhesus Monkeys	Hampton Court	Trials, errors	As many per day as would go	10 successive perfect trials	Food	Female 66 trials, male 113 trials	....
Yerkes & Huggins (1903).....	3 craw-fish	Triangle Maze, one corner open	Trials, errors	2 trials per day	Errorless trial	Escape	50 to 100 experiences necessary for perfect association	Habit persists after 10 days
Allen (1904).	6 guinea pigs	Simple rectangular maze	Time, distance	1	Correct path in 166 minutes	Food	Habit was formed	Retained 63 days without great loss
Porter (1906)	Sparrow	Hampton Court	Trials	1 trial per day	10 successive perfect trials	Food	Perfect trial in 23 days failed on 10 perfect successive runs	After 30 days no bird was perfect
Rouse (1906)	6 pigeons	Modified Hampton	Time	1	Perfect response	Food	Gradual decrease in time	Association perfect for several weeks
Yoakum (1909).....	2 squirrels	Hampton Court Maze	Time, trials	More than one	Eliminate errors, decrease time	Food	No. 1 ran maze in .35 minutes, No. 4 in .28 minutes	Habit easily upset

Experimenter	Animal	Maze	Record	Trials per day	Criterion of Learning	Stimulus	Results	Retention
Hunter (1911).....	8 pigeons	3 modified Hampton Mazes	Errors, trials, time	3	Perfect automatic trial	Food	Maze A trials 10–15 perfect Maze B 38th and 60th trials perfect Maze C results indefinite	Practically perfect for 4 weeks (no intervening training)
Thorndike (1911).....	1 fish (Fundulus)	Tank with glass partitions	Description	Several	Perfect habit	Shade	Learns to avoid stops	....
Yerkes (....)	1 turtle	Simple box, three partitions	Time, errors, trials	Every two hours	Perfect trial	Nest	30th trial perfect, 40 seconds	....
Yerkes (1912)	1 earthworm	T shaped maze	Error, time, trials	5 to 20	Several perfect series	Punishment: sand-paper, salt solution, electricity	Trials 783 to 788 perfect	70 trials in the opposite maze failed to break direction habit
Hicks & Carr (1912).....	4 adult humans, 5 children, 23 rats	Modified Hampton Court (9 blind alleys)	Time, error, distance	1	Humans: 1 perfect trial Rats: 2 perfect trials	Release Rats: food	Adults 11.25 ± 3.9 Trials Children 11 ± 2 Trials Rats 11.4 ± 4.1 Trials	....

Experimenter	Animal	Maze	Record	Trials per day	Criterion of Learning	Stimulus	Results	Retention
Turner (1914)	Roach	Simple rectangular maze	Trials	Intervals of half hour until learned	Perfect trial	Punishment	Habit formed	Effects of training persist long time; marked lapse after 12 hours
Sackett (1915).....	3 porcupines	Hampton Court	Trials, time, errors	No. 6 as many times as would go Nos. 3, 4 twice daily	10 successive perfect trials	Food	No. 6—48 trials No. 4—27 trials No. 3—91 trials to complete learning	Some loss after 10 days
Schwartz & Safir (1915)	10 fiddler crabs	Triangular maze like Yerkes'	Trials, time	As many as animal will make	Taking most direct path	Escape into burrow	Learned to take direct path	Persists after 10 days, can be unlearned
Churchill (1916).....	8 goldfish	Tank with 3 compartments	Gross time, trials	1	Perfect habit	Food	Group I—(4 fish) 2 min., 36 trials Group II—(2 fish) 1 min., 20 trials Group III—(2 fish) 1 min., 36 trials	Fairly retained after 13 days
Thompson (1917).....	3 snails	'U' shaped labrynth	Trials	Every other day as many as would make	Perfect habit	Escape to air	No indication of learning	....

adopted, so that it will be possible to say in an arbitrary way at least how difficult a maze an animal can learn. In 1919 Stetson and Dashiell proposed a multiple unit system of maze construction which, they suggested, would make maze situations more comparable. They also claimed the readiness with which the parts of these mazes can be redistributed makes it easier to isolate various factors which appear to be changing the results with reference to particular conditions of the maze. Using such a maze it would perhaps be possible to determine for any one species of animals the amount of effort which must be expended to learn each additional unit by having one group learn one unit, another two units, a third three units, etc.; then the results obtained in using another species of animals upon these same units could be interpreted in relation to the above species.

Studies of Retention

The above table also shows the results of some tests of retention, but these tests have been incidental to the main problem. Brockbank (1919) records the results of the first specific study of retention in a paper called "Redintegration in the Albino Rat." He trained groups of animals by the method of one trial per day or three trials per day, as the case might be, on the Watson circular maze, until they had completed six successive perfect trials, after which he continued the training for nine trials. Some were tested after thirty days, some after forty-five, and others after seventy, to determine if the degree of perfection established during the learning still continued. He found that certain errors which he called dominant, because of their persistency during the first learning, were the first to re-appear, when redintegration tests were made, which he interprets as meaning that the integration at these points was less perfect. It would also appear that there was a greater amount of practice upon the points which were established earlier in the learning process. As an explanation of the cause of the dominant errors Brockbank only says that they are nearer the higher functional limits of the rat's organization than are other integrations required in the habit. Since the rats of any one group had an equal amount of previous practice, this explanation would not seem to be in accordance with his statement that there were individual differences in the position of the dominant error. Besides it is questionable whether it is possible to say that

the avoidance of one blind alley makes a greater functional requirement than the avoidance of another. The learning of a rope ladder problem during the intermission between learning and the retention tests did not interfere with the maze habit, and training in an inclined problem box previous to the maze learning facilitated the maze habit and also the retention of this habit. With the exception of these points nothing very definite is established with regard to retention. This is an important study in animal learning, because the learning ability of an animal may be best measured by his retentive power. This problem might be solved by determining the saving in the amount of effort as measured by time or distance, etc., in relearning a given problem after a period of no training. Questions to be answered are: (1) Does the animal which learns the problem with the least effort also relearn with the least effort, or does the animal which is slowest in the learning retain the most? (2) Is this retention of a general or of a specific nature? (3) Is it proportionate to the amount of learning as measured by the effort required? etc. The second question has been dealt with in experiments upon transfer of training which will be discussed later.

Sensory Field Most Essential in Maze Learning

Watson (1907) using the Hampton Court maze attempted to solve the problem of which sensory field is most important to the rat in maze learning by what may be called the method of elimination, with the following results: (1) Normal rats of Group I mastered the maze in an average of 30 trials, and these rats could after learning run the maze in the dark without change in reaction. (2) Normal rats trained in the dark learned the maze in an average of 30 trials also. (3) Normal rats after having mastered the maze were deprived of vision and twenty-five days later were able to react in a practically normal way. (4) Blind rats learned the maze in a normal number of trials. (5) Forty days after operation anosmic rats learned the maze in a normal number of trials, when tested in the light, but were slightly subnormal, when tested in darkness, which shows that with these rats vision may have been somewhat important. (6) Partially deaf rats learned in a normal number of trials. (7) Rats that had mastered the maze gave transient effect from the removal of vibrissæ, but could learn in normal time. (8) Collodium was put on the nose and feet of rats twenty-four hours before learning, but produced no effect.

(9) One rat, blind, anosmic and without vibrissæ, learned the maze in just a little longer than normal time. Having eliminated all other processes, he concludes that in learning the maze senses other than the kinesthetic and tactual can be dispensed with.

Vincent (1915) in four papers on "The White Rat and the Maze Problem" took the positive side of this problem and added to the ordinary Watson maze, which as she says is essentially non-sensory (being practically uniform in sensory stimulation), a sensory stimulus, in order to determine whether a rat, if given an assistance of this sort, would accept it and by this means learn to thread the labyrinth pathway in a fewer number of trials and less time than a rat in a normal maze. The visual control consisted of the true path being painted white, the cul-de-sacs black with one set of rats and with another set the opposite condition; the olfactory control consisted of a beef extract trail through the true path or the cul-de-sac, as the case might be; and a tactual control was inserted by constructing a maze with the sides down, so that the animal had to help himself along with his nose and vibrissæ. The initial accuracy of these rats was much greater than that of the normal (normal in this case refers to the maze; all the rats were normal, except one set from which the vibrissæ were removed), but the final accuracy was approached much more slowly, which was due, Miss Vincent thinks, to the fact that the controls, when once they became effective, had to be noted, so that the rats never attained the automatism that was attained by rats in the normal maze.

In a separate monograph Vincent (1912) discussed the *Function of the Vibrissæ in the Behavior of the White Rat.* She used the tactual maze described above with several groups of rats in an attempt to determine the function of the vibrissæ in learning. She found that normal animals could learn this maze as any other maze. With rats with no vibrissæ, however, there was an increase in errors, decrease in speed, slightly lengthened learning period and a great number of slips and falls with much incoördination in the whole behavior. Rats which had vibrissæ cut only on one side, learned the maze more rapidly than any other group, due to the fact that the rats kept to the side of the maze on which they had vibrissæ, and so failed to make some errors. She found that rats that lacked vision could offset this loss by means of tactual sensations from the vibrissæ, and those that had no vibrissæ could use

vision as an aid; but rats deprived of both these sense organs were unable to learn after prolonged training. She, also, found that the rat could make use of its vibrissæ in discriminating an alley with corrugated sides from two other alleys in a three-choice discrimination box, especially if punishment was introduced so that the rats made the discrimination more slowly.

Bogardus and Henke (1911) experimented to determine the part played by actual head and nose contact in learning the maze and found that the number of such contacts is much larger at the beginning of the learning and decreases in the same proportion as the number of errors. They conclude that tactual sensations are of first importance in *learning* the maze.

Carr (1917) in three papers called "Maze Studies with the White Rat" tested the effects of certain alterations before running the maze and while running the maze on a large number of normal, blind, and anosmic rats to determine "the dependence of the maze stability on the wider sensory environment in which it was developed." He considers that the experimental work discussed above has shown that the rat depends chiefly upon the tactual and kinesthetic sense in the beginning of learning a maze, and principally on kinesthetic after the learning is completed. By introducing a change in location of the cage, rotation of the maze, dizziness just before running, variations in route to the cage, etc., he sought to show, whether or not the learning is in reference to the conditions of the animals' entire environment. He concluded with the experimenters named above that the maze habit consists essentially of a tactual kinesthetic motor coördination, but is dependent upon a wider sensory situation which is mediated through vision and olfaction. Vision has a stimulating effect either due to the tonic effect of such sensations; or the bad effects of the operation upon the rats deprived of vision. Vision has, however, a distracting effect which may occur even after the habit has become automatic, if important factors in the total visual situation are changed. The adaptive power of the anosmic rat is practically equal to the normal, but that of the blind rat is much inferior to both of the above groups, which would indicate that, for the rat, not the number but the kind of sense organs is the important thing.

All the evidence, which has accumulated upon the senses used in maze learning, is consistent in showing that the principal sense organs are tactual and kinesthetic, while other sense organs have

supplementary effects. The rat is able to make adaptations for the loss of these supplementary senses, and even in some amount for the tactual. This is probably true of any problem that is as essentially motor as the maze. It would be interesting to try similar experimentation upon the inclined plane problem box where at the beginning the problem seems to be more essentially visual.

Lashley and Franz (1917) and Lashley (1919) reported the effects of various operations upon the brain of the white rat, on the ability to form simple habits and the retention of such habits. They found that a rat which had acquired a simple maze habit could retain it after the destruction of the cortex above and in front of the knee of the corpus callosum; but in the case of the problem box, complete destruction of the frontal poles causes the loss of the habit. Some animals were able to reacquire it, however, even when there was nearly complete destruction of the frontal poles. Lashley further showed that learning at the normal rate of either kinesthetic or visuo-motor habits is possible after the destruction of any given part of the cerebral cortex of the rat. The correlation of structure with function presents one of the most interesting problems for animal behaviorists, and it is quite possible that some of the problems in learning, in which the factors seem particularly difficult to define, can best be approached in this way.

Another method of eliminating factors is to remove the possibility of their expression. Hunter (1920) attacked the sense organ problem from this point of view. His purpose was to discover what the white rat can do with the kinesthetic sense which seems so all-important to him, if the ability to use the other sense-organs is excluded by the limitations of the problem. Previous to this study Carr (1917) had reported his results from a problem on simple alternation with the white rat. The problem grew out of an attempt to investigate the forming of position habits so common in discrimination problems. Such a problem he says presents the following aspects of interest: "(1) The determination of limits of complexity which a given animal can master; (2) The relative difficulty of sequences differing in kind and degree of complexity; (3) The possibility of discovering new aspects of the learning process; (4) The determination of the various conditions conducive to the development of such habits; and (5) the character of the sensori-motor mechanisms involved in such series of alternating habits." He approached the solution of this problem by training

8 white rats to choose one path instead of another in the series of RLRL, etc. In order to do this the rat must learn four things: to adapt its alternate choices to the given order of presentation; acquire the system of alternate choices; resist the tendency of developing a position preference; and learn to choose the initial trial of each day's test. All of the animals learned this problem with a high degree of proficiency and 85 per cent. correct responses were obtained by all in 600 trials. Considerable individual difference was shown, one rat mastering the problem in 168 trials. During the learning process the time between trials could be lengthened to 16.5 seconds without interfering with the habits. After learning this interval could be increased to about 60 seconds without the failure of the habit, and the rat could be taken out of the apparatus and placed in a novel situation for 50 seconds and still make about 70 per cent. correct responses. Carr concluded that the learning was controlled partly by the previous sensory experience and partly by the distinctive motor attitude which was held during the delay.

Carr and Koch (1919) reported a preliminary experiment, in which the rate of learning on the alternation problem of one group of rats, which was prevented from making an error, was compared with the rate of learning of a group, which was given "free learning." With the first group the possibility of errors was eliminated by putting an endstop in the wrong alley at the point of choice. Test series were given this group every 5 days to determine their ability to make the choice when errors were allowed. "Free learning" consisted in the animal's being permitted to make his own choice. Within the limits of this experiment (2000–3000 trials), neither group succeeded in learning the problem, but such results as were obtained favored the "free learning" group, indicating that the making of errors may be a factor, which is of value in learning a problem.

Hunter, in using this problem for the purpose of testing the kinesthetic sense, found that the simple alternation habit in a T-shaped maze was mastered in 10 to 180 trials with seven rats. Using the same apparatus he found it impossible to set up the habit of double alternation with any of ten rats in 550 trials. He then constructed an apparatus which he calls the temporal maze because only the time elements of the problem changed as opposed to modification of both time and space elements in the ordinary maze. After prolonged training no rat was able to solve the double alternation

problem in this apparatus, even the six rats which had previously mastered a spatial maze, in which the turns were so arranged as to give a double alternation sequence. Hunter suggests that the reason for this is probably due to the fact that, "the experience of running around the left side of the *T* (kinesthetic, tactual, olfactory, etc.) can serve as a cue for going around the right side of the *T* or for going around the left side of the *T* again, but it cannot serve at one time for the first response and at another time for the second one." This problem as outlined by Carr above and as complicated by Hunter's facts with reference to the white rat seems to present a problem which would be excellent for comparative purposes. Several interesting problems in this connection present themselves for consideration: Could other animals learn the double alternation problem by the same methods? Could the rat learn the double alternation problem if a visual or sensory cue, such as Miss Vincent used, were added to one side of the maze? Here, there would be the same situation as Hunter notes above, the necessity for a stimulus acting at one time for a movement to the left and at another for a new movement to the right, *e.g.*, if a black-white maze was used the series would be BBWWBB, etc., but this would be in addition to the kinesthetic stimulus. Could a rat learn a maze which for explanatory purposes might be called a double *T*, in which it was required to run around the right arm of one, then the right arm of the other, then to the left on the first and the left on the second, etc.? It is possible also that further study of the temporal maze experiment might aid in the solution of the problem which I shall next consider, viz., what is the nature of the process which makes perfect learning possible?

Theories of Learning

Aside from the problem of an intelligence rating scale for animals, there is no other problem so important in animal learning as the factors that determine learning, and indeed some psychologists would insist that this is the most important problem in animal behavior. These scientists believe that the laws of learning can be studied in the lower animals unobstructed by the many complexities which overlie the basic principles in human learning. If these processes could be discovered, it would then be possible to regulate learning, by putting in factors which would be known to influence the processes concerned in learning. As I have said before

in this paper, although it may be quite possible to discover a great deal about the sensory processes of human beings from animal subjects, I consider it highly improbable that much can be found out about the human learning process in this way. In the case of the factors which determine learning, it seems fallacious to believe that such a factor in a simple relation can be put into a complex situation, and be unchanged by the situation. I would not, however, minimize the importance of this problem, for it is surely worth studying for its own sake, whether or not it adds any considerable data to human psychology. Enough has been done in animal behavior to show that the problem at best is very complicated, and a number of theories and a great deal of experimental work have been discussed already in attempting to explain, for example, the simple problem of how a white rat is able to eliminate one by one the blind alleys in a maze, and find its way from the entrance box to the food box by the most direct route.

When the assumption of a conscious directive force in animal learning was dismissed, the conception of some sort of pleasure-pain experience arose, and was until recently accepted as a logical explanation. Hoge, Stocking, Yerkes, Dodson, Cole, and others attempted to determine the relative merit of pain and pleasure, as the stimulus for learning; and Hubbert, Lashley, and Vincent to test the relation of the food stimulus in the maze to the elimination of errors. The results of this experimentation showed that the pleasure-pain theory was insufficient to account for the facts of learning. During this decade, hypotheses to cover these facts have been proposed by Carr, Watson, Peterson, Ulrich, *et al.*

The pleasure-pain theory of animal learning, as it was usually stated, simply assumed that the animal experienced pleasure when it was successful, and either the absence of pleasure or pain when it failed. These terms were made more behavioristic when they were changed to satisfaction-dissatisfaction, which was presumed to denote that the pleasure and pain were unconscious or physiological; but, because of the conditions which they established, acted as stamping-in or eliminating processes, respectively. This caused certain experiences to be maintained and others excluded. Such a process was said, from the physiologist's point of view, to depend upon such changes, as the increase in bodily tonus which followed the success; or an anabolic process arising from the pleasure, and a catabolic process arising from displeasure; or as recently suggested

by Dunlap (1917) to changes brought about by the secretions of the ductless glands (or from some tissue whose primary function is not secretion), in such a way as to make such chemical changes in certain parts of the nervous system that the arcs then in use will be fixed, etc.

Such a conception probably arose from the fact that in animal experimentation it has always been necessary to introduce a stimulus which depends upon a primitive mechanism common to all animals, namely, the desire for food and the avoidance of pain. It is obviously impossible to direct animal subjects as one can human subjects, by ideas. Some consideration has been given to the question of the superiority of pain or pleasure as a stimulus. As far as practice is concerned each experimenter has determined for himself what stimulus seemed to give the best results. On the whole, food has been used most often, but Yerkes (1908) in discussing his experiments with the dancing mouse protests against this method and says, "The desire for food is unsatisfactory as a motive in animal behavior work, first, because a condition of utter hunger is unfavorable for the performance of complex acts; second, because it is impossible to control the strength of the motive, and finally, because it is an inhumane method of experimentation. In general, the method of punishment is more satisfactory than the method of reward, because it can be controlled to a greater extent." It would seem practically impossible to use punishment alone as a stimulus, because, unless rats are somewhat hungry, it is often impossible to get them to perform with the same activity that they otherwise show; also punishment seems to me as variable a factor as food, for in some recent experimental work, I found that some animals seemed far more sensitive to a certain amount of punishment than others.

Hoge and Stocking (1912) reported the results of an experiment on the "Relative Value of Punishment and Reward as Motives." Six rats were trained in brightness discrimination, two of them were fed after the correct response, two were punished for an incorrect response with a slight electric shock, and with the other two a combination of these two stimuli was used. Both rats of the last group completed the learning before any of the others in an average of 520 trials. One rat of Group II learned the problem in 550 trials but the other rat of this group had not learned it in 620 trials, when the experiment was discontinued. Neither rat of Group I learned

the problem in 590 trials. The authors conclude, therefore, that a combination of reward and punishment motives is the more effective in bringing about visual discrimination than either motive used alone.

Previous to this time Yerkes and Dodson (1908) and Cole (1911) had experimented to discover the relation of the strength of stimulus to the rate of learning in the dancing mouse and in the chick respectively. The stimulus was an electric shock which was increased by a constant amount. Both found that with easy discriminations the rate of learning was more rapid, the stronger the stimulus. For Cole the same result held for a discrimination defined by him as of medium difficulty. When the discrimination became very difficult Yerkes and Dodson found that the learning increased rapidly, as the strength of the stimulus is increased, up to a certain optimal point and then it began to decrease. Cole found, however, that if the records of the chicks which failed entirely on the difficult discriminations were eliminated, the same law which he found for easy and medium discriminations held good here also. Hoge and Stocking did not make use of these facts in studying the relative value of reward and punishment for they did not determine an optimal stimulus for the discrimination which they were demanding.

Dodson (1917) approached the problem from another angle. He used the same problem of brightness discrimination and introduced four different strengths of shock: 60, 75, 115, and 150 units respectively, and 4 degrees of hunger: 24, 31, 41, and 48 hours between feeding and experiment respectively. He found that the learning was facilitated by an increase in stimulus up to a certain point and after that learning was hindered in the case of punishment, by the distraction produced by the excitement caused by a severe stimulus; and in the case of food, by apparent loss of hunger. He found the most favorable intensity of the shock to be 75 units and the most favorable intensity of starvation to be 41 hours for animals 78 days old. The shock produced learning at a more rapid rate than the hunger. This might be true, it seems to me, not because one factor is reward and the other punishment, for an attempt to secure food after starvation may be looked upon as the desire to avoid pain as well as the avoidance of an electrical shock, but in the latter case the punishment would seem to have a closer connection with the problem to be solved. These experiments

have been performed with reference to tests in discrimination and perhaps the results can not be used with the maze and problem box situations which present a different condition. In any case food has been used almost entirely as a reward for the correct solution of these problems. In the maze, at least, the stimulus is somewhat complicated by the emotional factor of imprisonment as well as by curiosity, timidity, etc., which function in all tests.

To determine the influence of the food factors in the maze situation there have been various studies on the elimination of the cul-de-sacs, in order to discover whether or not they were eliminated by the animals from the food backward. To ascertain the effect of the food as a stimulus, this problem was first investigated by Vincent (1915) using the data which she had obtained from her research on the effects of various sensory controls on learning. She compared the total scores of the blind alleys 1, 2, and 3 with the corresponding error scores for alleys 5, 6, and 7 in the Hampton Court maze to determine whether the animals would make a larger number of errors in the first three alleys than in the last three. She found this to be the case with the records from the normal maze, the combined black-white maze and the cutaneous normal maze. In the open maze at the beginning of learning the first alleys have the smaller score. This is due, Miss Vincent thinks, to the fact that the animals were sometimes able to jump across from blind alleys 4 and 5 into the food box. In this maze, however, when perfect automatism had been established the number of errors was greater in the last three cul-de-sacs than in the first three. In the olfactory maze the incidental errors increased toward the end of the series, probably because when the trail was in the blind alleys, the cul-de-sacs, especially at the beginning of the run, were very attractive. The last three counts are lower, however, for 5, 6, and 7 in this experiment also. Such evidence would show that while sensory cues may influence the distribution of errors there is still a tendency to eliminate the final blind alleys first.

Hubbert (1915) obtained somewhat different results when (with reference to this point) she studied her data upon the age differences in rats. She counted three types of errors, namely, (1) taking the wrong turn at the entrance of the alley, (2) going past the entrance of the next alley and (3) retracing on the true path toward an alley after having made the correct turn. She excluded alley number 6 because these three possibilities were not present, and alley number

1, because of the emotional factors of timidity and curiosity which she thought were more potent in the first alley. With the 25 day rats, she found that on the average there was no uniform progression but 4 and 5 were eliminated before 2 and 3; with the 65 day rats the values for 3, 4, and 5 were so nearly identical that the results would not warrant an interpretation; with the 200 day rats 4 and 5 were not eliminated before 2 and 3 and with the 300 day rats the counts for alleys 3, 4, and 5 were almost identical. She concluded that, "from these experiments it seems fairly probable that the rapidity with which a given coördination in a complex habit is formed is not proportional to the distance from the point at which the coördination takes place to the point at which food is to be obtained."

In 1914 Hubbert and Lashley studied this problem with a somewhat different emphasis. They trained 56 rats on the Watson circular maze and counted the errors, which they divided into two types. Type I consisted of passing the doorway, and type II of making the wrong turn after the doorway was entered. The errors under Type I were found to be eliminated in less than two thirds as many trials as Type II, due to the fact, probably, that the animals are inclined to enter a doorway when they come to it rather than to pass by. This Hubbert and Lashley found to be true in three quarters of the cases. The errors of Type I were found to be eliminated serially; but the experimenters did not interpret this as a proof of retroactive association, because the process is probably complicated (1) by transfer of training from one part of the maze to another, (2) by orientation toward the center of the maze, or (3) by reactions to the curvatures of the passages. Serial elimination was not found in the case of errors of Type II, and Hubbert and Lashley concluded that the elimination of errors in the maze is not the formation of a series of conditional reflexes with reference to the food, which they had previously thought might be the case.

Carr (1917) criticized Watson's conclusions (which were established on the basis of Miss Hubbert's data) that since the blind alleys are not eliminated in the order of their nearness to the food box they can not be considered as a selective agency. Carr considers that this conclusion is not valid, because there would be perfect backward elimination only in case the food were the *sole* causal agency. He found by calculating Hubbert's data that 80 per cent.

of her rats eliminated the 6th error before the 5th, 50 per cent. the 5th before the 4th, 47 per cent. the 4th before the 3d, 73 per cent. the 3d before the 2d, and 70 per cent. the 2d before the 1st. Furthermore, the temporal order in which the cul-de-sacs are entered is not the same as their spatial order, and this factor must be taken into consideration. Carr, using the data from 8 different mazes, found that there is a tendency on the part of the rats to return to the point of entrance, which accounts for the increase in the number of errors in the first cul-de-sacs, and as demonstrated by Miss Vincent the sensory character of the maze determines the persistent entrance into certain cul-de-sacs. He concluded that "the amount of returning will vary with the animal, the maze, the stage of mastery, and the section of the maze" and that the character of the motives will change with different stages of mastery.

This interpretation was in harmony with his theory, as to what should constitute the selective principles of learning, which he advanced in 1914. He divides all animal problems into three classes: (1) Those composed of a series of simple acts, the final one of which is a successful reaction toward the stimulus, as illustrated by a simple problem box; (2) Those in which the successful act is complex, and can not be learned until a number of intervening unsuccessful movements are eliminated, for example, a maze or complex problem box; (3) Those which deal with the inhibition of some instinct or habit. That these problems are finally learned is due to certain selective principles called by Carr "frequency," "recency" and "vividness." In the first class of problems all three principles are effective, the successful act is the most frequent, the most recent and is followed by certain sensory consequences, which are more intense than those of any other act, because of the food stimulus and the completion of the problem. In the second type of problem the factors of frequency and intensity are the important ones. The segments of the true path are traversed more frequently than the cul-de-sacs, and the true path presents fewer obstacles to motor activity, rapidity, freedom, food, etc., therefore it is more vivid. In the third type of problem, recency and intensity are the effective factors and the inhibition of a habit or instinct is not due to the fact that they are eliminated, but to the fact that other movements become more important because of the intensifying effect of the stimulus and can be more easily aroused because of the recency. "The successful act is selected because it finally becomes the most

prepotent in the group; all others are eliminated, or better are 'suppressed,' because of their lesser development in functional efficiency." In concluding his discussion Carr does not contend that these are the only possible principles nor that they explain all aspects of the learning process, but he does believe that the real selective principles are objective ones and not subjective as pleasure and pain. He also maintains that the two theories can not be combined, for if pleasure and pain are the causal factors, then the recency, frequency, and intensity can only determine the rate of learning; but if they are the causal factors, pleasure and pain must be excluded as causally important.

Watson in his textbook, *Behavior, an Introduction to Comparative Psychology*, published in 1914 discusses the problem of fixation of arcs in habit at some length. In criticizing the conception that pleasure is the important element in fixing an arc, he says that there are three current misconceptions with regard to this theory: (1) "It has been assumed without further ado that the successful act is pleasant and that the unsuccessful act is unpleasant." With reference to this point he discusses Glaser's experiment on the formation of habits at high speed, in which rats dropped into a maze filled with hot or cold water formed the habit and reached the exit after making the correct turns. Watson believes that if the rat was forced upon coming out of such a maze to enter a hot box which he ordinarily would avoid he would still form the habit, because pleasure and pain of the final act have no influence upon such learning. In 1916 he performed an experiment designed to further the solution of this point. He used a simple problem box in which a covered food dish was placed. This cover could be operated by the experimenter, so that it might be kept over the dish for a varying length of time. One group of rats obtained food immediately upon entering the box, but another group were delayed for thirty seconds during which time they went through many and varied movements. These movements, however, were not effective in destroying the habit, and it was learned by the second group of rats as soon as by the first. This experiment caused Watson to conclude that the satisfaction of obtaining food was not a selective factor in the learning, because, if that were the case, the later random movements in the box would be those most benefited by the pleasure-giving stimulus rather than the more remote movements of attaining entrance, and this was not the case. Watson's second criticism of the alge-

donic theory is (2) it has assumed that it is always the successful act alone that is fixed. He illustrated his objections to this by observations of Basset and Ulrich to the effect that some animals in learning a problem box never succeeded in eliminating certain unsuccessful movements (persistent errors), even when the number of trials was increased considerably. Watson's third criticism is directed (3) against the assumption that there is a diffusion of nervous energy which can spread without passing through "preformed neural channels." He says "that the nervous system is not built to permit such functions. When a stimulus arises in a receptor there is just as orderly a progression of events then as later, when the habit is formed, viz., the stimulus is carried off along preformed and definite arcs to the effectors in the order in which the arcs offer the least resistance to the passage of the current. This order may vary with variations in the sum of intra- and extra-organic stimulation. There is no formation of new pathways." The change, which is brought about, is the functioning of a new set of arcs together. Watson then justifies the use of two of Carr's principles, viz., frequency and recency, but excludes vividness. Frequency alone, he thinks, will account for maze learning. For this purpose Watson presents the following argument: "Let *A* and *B* represent the segments of the true pathway and *X* the entrance to any cul-de-sac (let the segments be chosen somewhere in the middle of the maze). We will suppose that the animal is on its way to the position *A* for the first time. The chances of entering *B* and *X* are equal in the long run when the animal is in the position *A*. If the animal goes into the cul-de-sac *X*, and follows it out to the bitter end it must return. When it reaches the position of the letter *X* on the return" (the point of choice) "again the chances of entering *B* and *A* are equal. We thus see that there is a greater probability of the animal's remaining on the true pathway than of his leaving it. Possibly the case can be more definitely presented if we ask for the probability that the animal takes the wrong path. In order that this may occur it must (1) choose the wrong path from *A* to *X* and (2) choose the wrong path from *X* to *A*. The probability of each wrong choice is $\frac{1}{2}$; the probability that both wrong choices be made is $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$. The complementary probability or the probability that it takes the right path is $\frac{3}{4}$; *i.e.*, the chances are three to one that the animal goes out through *B* rather than comes back to the starting point. This factor (frequency) alone is probably sufficient to account for the formation of the maze habit."

Joseph Peterson (1916) criticizes the hypotheses of both Carr and Watson and proposes "completeness of response" as an "explanation principle in learning." He states his theory briefly thus: "In the case of the maze problem the animal on entering a cul-de-sac—or any other path, in fact—responds at first more or less incompletely, because all the subordinate activities involved can not take place at once. If the animal's progress is soon checked in a blind alley the animal is not seriously nonplused. Certain elements of the general response are tending to drain into other alleys that may recently have been passed, thus partially dividing the animal's activity. These elements now prevail when the others are checked. Let us suppose that the correct path, *A*, has just been passed when the animal suddenly comes to the end of the cul-de-sac, *B*. The tendencies to respond to *A* are still surviving and now direct the impeded activity into this, the successful, path. If, on the other hand, the correct path had been chosen the first time the distracting impulses toward *B* would have become fainter and fainter as the animal proceeded into *A*, and would finally have faded away. The principle is not different when the complexity of the situation is increased. When the food is finally reached all the remaining delayed reactions, the tendencies, still persisting, to go into other alleys recently passed, are relaxed—the act as a whole is complete." He thinks that, whereas (as pointed out in the quotation) recency and intensity are only valid in case of a number of neural tracts acting together, completeness of response is an explanatory principle either for a simple or a complex act. Frequency, while it may account for the animal's getting through the maze, will not account for perfect learning for there would always be a tendency for the animal to enter a blind alley, even after a large number of trials, if frequency alone were the principle of selection.

In order to establish these points Peterson (1917) did some experimental work on the "Effect of the Length of Blind Alleys on Maze Learning." He thought that if he could establish length of the alley as a function of elimination, it would show that frequency was at least not the only factor and perhaps an unimportant one. He says, "Mere probability explains truly enough how the animal gets to the food each time, but *that is not the problem of learning;* it does *not* explain how it happens that on the whole the second trial is better than the first, the third better than the second,

and so on. Frequency based on probability does not bring such a result: *it fails utterly to explain learning*, even in the simple case of the maze." Peterson worked this out on a hypothetical case of a rat in a maze with six cul-de-sacs, whose choice was determined at each entrance by the flipping of a coin. After accumulating considerable data by this method, he found, that if frequency or recency or both were the determining factors of the subsequent choices, the maze would never be learned. In real experimental work he trained Group I consisting of 16 white rats on two mazes, which he termed the *B* mazes and Group II consisting of 8 white rats on two different mazes called the *A* mazes. Mazes I *B* and I *A* were both constructed from a single maze by partitions and had the same food box. II *B* and II *A* were only different from the originals by having the alleys shortened. Both time and errors were used as criteria. The errors were divided according to degree and were marked *complete* (going to the stop in the alley), *half* (going approximately halfway), and *start* (turning into blind alley but not continuing a sufficient distance for the whole length of the body to be included). He found (1) a rapid decrease in the proportion of the returns to forward runs, (2) there is a change in the *nature* of the response to blind alleys from complete entrance progressively to elimination, (3) when a cul-de-sac was shortened it was eliminated more quickly than when it was full length. Peterson believes that frequency and recency are wholly inadequate to account for the specific kinds of results found in this experiment. The principle of intensity, he thinks, needs re-interpretation. He says, "when several stimuli act on an animal bringing about a series of responses as in this case, the final one of which is the successful one, it appears that somehow, not well understood yet, the various effects of these stimuli hold over into that of the final stimulus and that all together simultaneously act to direct the energy of the animal into the most consistent channels. In the large, these channels offer the least resistance and afford the most complete response."

Both Peterson's particular and general conclusions with regard to the effect of the length of blind alleys in maze running are borne out by further experimentation. De Camp (1920) studied "Relative Distance as a Factor in the White Rat's Selection of a Path." He used two mazes, a circular one and a rectangular one. Both consisted essentially of an entrance box from which a runway extended in either direction. In each path a food box was inserted

which was adjustable for different distances from the entrance box. One box was kept closed during any one set of trials and the other contained food. Six white rats were used as subjects. At the beginning of the experiment the difference in the two distances from the food box to the entrance box was made relatively large, and the rat was taught to go by the nearer one for food. About 10 or 15 trials were given a day during which time the boxes were kept constant, but on different days they were alternated so that the rat was not influenced by an association between the food and the direction of the turn. When the animal had learned to choose the shorter path, the distance between the two food boxes was gradually decreased, but the rats succeeded in choosing the shorter path until the difference between the two was very small. De Camp concludes: "It is clearly shown that the length of path is a factor that may influence the rat's course. Other things equal, the shorter of two paths will be traversed more and more until it becomes the usual one, provided a certain fractional difference in length exists between short and long paths. Our results indicate that this fractional difference is as low as 1/10, and in some cases lower. This means that of two paths leading to the same goal one of which is n feet long and the other $1.1n$ feet, after repeated trials, the former will be generally traveled, and it will be thus favored because it is shorter than the other."

That frequency is not a good explanatory principle for maze learning is borne out by experimentation of Dashiell (1920). Ten mazes were constructed all alike in certain essentials, viz., there was a blind alley on the right and one on the left of the true path and one straight ahead (R, *L*, and *F* respectively). Fifty white rats were used as subjects, five being used in each maze for one trial only. The empirical findings are: "(1) A blind alley opening straight ahead is more likely to be entered than to be passed, in about the ratio of 5 to 3; (2) a blind alley opening at the side is about as likely to be entered as to be passed; (3) the exit from a blind alley opening straight ahead is more likely to be in the forward than in the reverse or backward direction in about the ratio of 3 to 2; (4) the exit from a blind alley opening at the side is more likely to be in the forward than in the reverse direction, in about the ratio of 3½ to 1."

The author then asks certain questions to be solved in an analyzation of the maze problem. "(1) What is the potency of a

blind alley opening on a side of the runway and just preceding a turn in the true path in the same direction, as compared with a blind alley just following such a turn or one on the other side of the runway from a turn, etc.? (2) Will a given number of blind alleys offer more difficulties to the learning subject if placed simultaneously in the maze or serially? (3) Does the difficulty of a maze as a whole increase progressively with the number of blind alleys offered? If so, what type of mathematical progression obtains? (4) Is there any change in relative difficulty of different parts of a maze as the subject's trials increase in number? (5) To what extent would the findings on these or other similar questions be dependent upon the particular species and age of subjects used?"

Wiltbank (1919) criticized Peterson's theory of complete response as an explanation of learning. He says there are three difficulties in regard to Peterson's statement that when a rat came to a doorway there was a tendency to choose either path, and if he chose the blind that on emerging from it, the holdover effect from the recent tendency to proceed on the true path would now direct the activity upon the true path. Wiltbank objects to this (1) because there are a number of influences within the blind alley itself (butting into endstop, movement in and out of alley, direction with reference to the true path, etc.) which "may exert a modifying influence on the tendency to follow the true path," (2) because Peterson does not make clear how "when the animal is drawn back into the true path, the erroneous tendency is directed into the successful tendency as a result of a collision between them," (3) because it is difficult to understand how a tendency which had insufficient strength to keep an animal out of the blind alley could become reënforced during the incorrect response and prevent its return into the blind alley. Using some experimental data from his monograph on transfer of training, Wiltbank found that the learning of maze *D* aided in the relearning of maze *E* which had been partially learned before training on *D* was begun. He concludes from this that the complete response method was not efficacious or the relearning of *E* would have been inhibited by the learning of *D*, and he agrees with Holmes that "in behavior of the trial and error type, success is attained not by direct adaptive reaction, but by checking or reversing all the reactions except the right ones." Since in the case of the blind alley, the inhibition may become effective before the animal reaches the endstop, this would account for progressive elimination of errors.

Ulrich (1920) has recently published serially a long monograph called *Integration of Movements in Learning in the Albino Rat; A Study of the Adjustment of an Organism to its Environment.* Because of the length of this paper no adequate review can be given here of the entire experimental procedure. His general method was to study the reactions of white rats in various problems (latch box, maze, rope ladder, etc.) in an attempt to show "by an investigation of the integrated movements in learning in the white rat: (1) that learning begins from the moment the rat is placed in the box for preliminary feeding; (2) that all movements at all times are of equal importance in solving a problem, and that they are fundamental, inherent, integrated movements conjoined, or coördinated; (3) that several conjoined movements are used in an exaggerated manner to solve a problem, and a greater development of interaction of body parts is necessary for an exaggerated performance of these movements; (4) that no definite movements are retained, but, through training, these movements are performed with greater facility as is the case with any exercised parts of an animal's body; and (5) that solution of a problem does not occur because an animal responds 'successfully' after making repeated 'efforts' to do something." He studied the posturing and the number and kind of reflexes involved in learning each problem, and tested the rats for their reflex excitability. The treatment of the data partakes more of the nature of a general description than of statistical results. His general position with regard to the learning process is that learning does not come about through the establishment of new paths in the nervous system (here, he agrees with Watson) but is due to the facilitation of certain important movements by practice. Learning, he says, is not the establishment of a habit, but a greater facilitation of parts of the rat's body. It seems to me that "habit" as it is generally used means merely this.

This, then, is a statement of the principal theories of learning advanced by psychologists during this decade. There is no hypothesis that is generally accepted. The problem is very difficult because the factors are so subtle and the only way to approach it is just in the way it has been done: for one scientist to propose a theory and then to test that theory as to whether or not it will include the facts. If it will not, then it must be revised or discarded.

Distribution of Effort in Learning

In the experiments included under this heading, certain experimenters proposed to see what effects the introduction of various methods, controls, etc., would have upon the learning process. Ulrich (1915) tested the change in learning due to the distribution of effort over one or more trials per day. White rats were used as subjects. In the first part of the experiment the latch box was the problem. Group I consisting of 17 rats were given one trial daily; Group II of 11 rats was given three trials, and Group III of 11 rats five trials daily. The problem was considered solved when the rats reached a norm of one second per trial and held it for two successive days, regardless of the fact whether they were running 1 trial, 3 trials or 5 trials a day. It would seem that the criterion of learning was more difficult for the rats which ran more trials per day. The results of this test were as follows: Learning was more economical when it was distributed, for fewer trials were required with the rats that only one trial per day, and the greatest number with those that ran five trials per day. On the other hand, those rats that ran fewer trials per day required more days for the learning. After these facts were established Ulrich then decided to train another group of rats with one trial every other day, and one with a trial every third day to see if greater distribution was still more efficient. With each of these groups the first rat finished in 9 days as opposed to 11 days for the one trial a day group. The next step in the experimentation was to repeat the problem using the circular maze as the problem. Here, the same general results were found. "The first rat of the one trial group completed learning in 14 trials, that of the three and five trial groups in 39 and 50 trials, respectively. On the other hand, the one, three, and five trial groups required 14, 13, and 10 days, respectively, to learn the problem." The above results were likewise obtained when three problems, the inclined plane, the latch box, and the maze, were learned abreast. Two possible explanations are given as the reason for these results, (1) One trial a day may be more efficient, because more trials produce fatigue, which hinders the learning rather than helps it. Ulrich does not believe this is a factor, for the rats were apparently as active in the later trials given on any one day as on the first; and the last trial often required less time than the others; (2) Several trials produce too pronounced metabolic changes. This explanation seems nearer the truth to Ulrich.

He says, "It is possible that with one trial a day, or at greater intervals, just sufficient transformation of energy occurs to make every trial physiologically effective, whereas, when several trials are given concurrently, there is a too rapid and extensive transformation of energy."

Lashley (1917) examined the distance records of 56 rats trained by the method of 5 trials per day on the circular maze to determine what the causal factor in the relation of the distribution of practice to the rate of learning might be. He tabulated the number of duplicate and diverse errors in the last trial in each day's practice and of the first on the succeeding day and compared that with the number of diverse and duplicate errors in succeeding trials, when the two sets had an equal number of errors. He was able to find 175 of such and found (1) in both kinds of pairs the duplicate errors exceed the diverse ones by a small amount, (2) 49.05 per cent. of the errors made in successive periods of practice were diverse while in successive trials made on the same day 44.48 per cent. of the errors were diverse, (3) in the trials separated by a 24-hour interval, therefore, there were 10 per cent. fewer duplicate errors. This bears out the hypothesis that persisting errors are the probable cause of the results obtained with different periods of practice. Lashley says two questions still remain to be answered: "first is this the only factor involved in producing the results noted when different distributions of practice are used; second, is the hypothesis applicable to other types of activity than motor forms such as archery and typewriting?"

Pechstein (1917) made an extensive study of the whole and part methods in learning a maze to determine the relative efficiency of these two methods. During the experimentation, 177 white rats and 112 humans of sophomore university standing were trained on a modified Hampton Court maze which could be divided into four parts so that each could be learned separately. From each part when used by itself the subject could enter directly into the food box which was in the center. The pattern of the rat maze was duplicated for the humans. The human subjects were totally unfamiliar with the maze problem and the experimenter used as nearly as possible an entirely objective method with them, except they were given verbal directions. Learning by the part method consisted of learning each section of the maze separately, and then combining them until the whole path could be traversed without

error as in the whole method. With this method, Pechstein found that the rats learned in slightly fewer number of trials by the whole method but made more errors and required a longer time. With human subjects the whole method was more efficient as judged by all three criteria. Since the large number of errors with the rats was found to be due largely to retracing, and to the entering of cul-de-sacs on this backward progression, Pechstein determined to study the influence of the prevention of returns. He found that when no more retracing was allowed with whole method learning than was possible with part method, both rats and humans found the whole method much more efficient. Pechstein then experimented to see if the part method could be modified in such a way as to become efficient. Four modifications were tested: (1) Direct Repetitive—The subject was required to learn Section I and then I, II (II not having been learned separately), then I, II, III and finally I, II, III, IV. (2) Reversed Repetitive—Here the same method was used as above except the subject learned Section IV first, then IV, III, etc., backwards and after this learning was completed he was required to run the maze in the order I, II, III, IV. (3) Progressive Part—The subject learned Section I, then Section II separately, then combined the two, then Section III separately, then ran I, II, III and finally learned Section IV and ran I,II, III, IV. (4) Elaborative Part—Here, the subjects (all rats in this case) mastered each section of the maze and then were required to combine two a day in a various succession of pairs. On the sixth day they ran the entire maze in the reverse order and the next day in the correct order. In every case the modified part method was superior to either the whole or part methods, due, Pechstein thinks, to the effects of transfer which can function here to its full extent and to the progressive and distributive handling they furnish to the positional factors. Massed effort was more efficacious in the case of the part methods than with the whole but with the whole method there was improvement with massed effort after the learning had progressed well toward mastery. This leads Pechstein to say that "the full significance of the distribution of the learning effort is far from being known." He believes that the difficulty of the problem, the stage of the learning and the method of learning all effect this factor.

Dashiell (1920) compared the complete versus the alternate method of learning two habits. He proposed to find out whether

it was more economical for a subject to learn two habits by learning first one completely and then the other or to learn them "alongside" each other. The experiment was divided into six parts as follows: I—Maze running by 6 white rats; II—Maze running by 8 kindergarten children; III—Maze running (pencil maze) by 4 adults; IV—Card sorting by 4 adults; V—Adding by 10 adults. The same general method was used throughout. One half of the subjects on any one problem were trained until they had perfected a certain habit and then were set upon a similar habit until they had completely learned it. The other subjects were trained on first one habit and then on the other until they had completely learned both. In the case of the maze problems the two habits were the exact opposites of each other. The results show that in every case the complete method of learning was more economical than the alternate method as measured by (*a*) the number of trials necessary for learning, (*b*) regularity in improvement, (*c*) average amount of scores made on individual trials, (*d*) the rate of acceleration of improvement.

The distribution of effort between successive trials has been investigated by Carr, Freeman and Yarbrough. Carr (1919) studied the effect of the length of the time interval in successive association upon the mastery of the alternation problem (described above). Miss Lewis, a graduate student, experimented to determine the effect on learning of groups *A*, *B*, and *C*, using 15, 25, and 35 seconds, respectively, as the interval between trials. Whenever the condition of the animal warranted it, 20 trials were given per day until they had made 90 per cent. correct choices in 100 trials. Carr concluded that so far as the evidence showed the differences employed in the experiment were without effect. The second part of the experiment was conducted by Carr. Groups *A*, *B*, and *C*, consisting of 6 rats each, were tested for the intervals 5, 10, and 15 seconds respectively. The experiment was discontinued before learning was complete but the records showed that the 10 second interval seemed slightly more effective. The third part of the experiment was conducted by Miss Koch. Seven rats were tested with the 10 second interval and six with the 15 second interval. But one path of the maze was open at a time so errors during the training were impossible. A test series was given every five days in which both paths were open in order to note the progress of the learning. Although there was in this case

some indication that the 10 second interval was the better, Carr concludes here also that there was no conclusive evidence to indicate that the time interval affected the learning.

Carr and Freeman (1919) present experimental data in order to further the solution of two problems, viz. (1) the relative merit of simultaneous and successive presentation in relation to the speed of learning and (2) the readiness with which a given temporal sequence will function in a backward as compared with a forward direction. The apparatus was similar to a T-shaped maze with zigzags in the outer alleys to prevent the animal from sensing from a distance whether or not the door opening to the food box was open or closed. White rats were taught to set up an association between an auditory stimulus made by an electric buzzer and turning back from the closed door as follows: group I, the buzzer was sounded while the rats were turning around which was termed simultaneous presentation; group II, the buzzer was sounded when the rats had reached a point in the alley somewhat over halfway toward the closed door; group III, the buzzer was sounded when the rats had retraced from the closed door to the point used with group II. A test series was given periodically to determine how effective the buzzer could be in causing all the animals to retrace their paths. In this test the buzzer was sounded at the same point for all three groups in any one test, but this point was varied from test to test, so that no position habit would be formed. The results showed that successive presentation was more favorable than simultaneous and no rat in the third group showed any signs of forming an association that would function in a backward direction. Yarbrough suggests two weaknesses in the experimental procedure, (1) there was not a constant time interval between the two terms to be associated since the rats would not traverse the distance from the point *P* to the closed door at the same speed each time, and (2) the buzzer which was the sound stimulus was in contact with the maze so that the rat may have been responding to a cutaneous or kinesthetic stimulus rather than to a sound stimulus.

Yarbrough (1921) studied "The Influence of the Time Interval Upon the Rate of Learning in the White Rat." He trained rats of one set to associate an auditory stimulus (buzzer) with pain (electric shock) and another set to associate visual (light) with auditory. The time interval between the two stimuli was increased from continuous stimulation to 1, 2, 4 and 6 second intervals, respectively,

with five groups of rats. The results were: "(1) Association in the backward direction is only slightly more difficult than in the forward direction when the terms to be associated are presented in immediate succession. This small difference is by no means conclusive. (2) It is more difficult to learn an association in the backward direction when the terms are separated by one second than when they are presented in immediate succession. (3) It is more difficult to associate a motor response with an auditory stimulus when the latter is presented after the former has occurred than when it is presented before the initiation of the motor response. (4) The data found do not warrant the statement that it is easier to associate a motor response with light than with sound when the stimuli are presented in immediate succession." He then tested the effects of simultaneous and successive presentation and found that although simultaneous presentation was slightly superior the superiority was not sufficient to be significant.

Transfer of Training

The problem of transfer of training may almost be said to be a problem of this decade. Previous to this time the problem had been mentioned by Yerkes, Thorndike, Richardson, and Yoakum, but no extended experimental work had been done. Since 1917, Webb, Wylie, and Wiltbank have published long monographs containing experimental results and discussions upon this topic. The problem of transfer has particular significance in the training of animals.

The first appearance of this problem in the present decade was in a paper by Hunter (1911) called "Some Labyrinth Habits of the Domestic Pigeon." Five pigeons, Nos. 1, 2, 5, 7, and 8, were trained on maze *B*. Of these 1, 2, and 7 had previously learned maze *A*, while 5 and 8 were new to maze training. The esulrts show that pigeons 5 and 8 learned the problem equally as soon as did 1, 2, and 7. The curves showing the results with these two groups revealed the fact that both the time and error curves of 1, 2, and 7 rose higher than that of 5 and 8, but the time curve for 1, 2, and 7 drops permanently sooner than that for 5 and 8. The author concludes (1) the habits acquired in labyrinth *A* by birds 1, 2, and 7 *interfered* with their learning of *B* and resulted in a slow elimination of errors; (2) the training in *A* made it possible for Nos. 1, 2, and 7 to reduce their time records permanently sooner

than did Nos. 5 and 8. (Hunter suggests that this may be due to the fact that training resulted in the birds acquiring an attitude similar to self-confidence in man.) Further results might have been obtained with reference to the inhibitory effect of the two mazes if 5 and 8 had been trained upon maze *A* after learning *B*. If more animals had been used and the same results obtained they might have pointed to a general transfer of attitude toward the maze situation even if the muscular habits established in the first maze were inhibitory to the setting up of certain other muscular habits required for the learning of the second maze.

Bogardus and Henke (1911), in connection with their problem on tactual sensations in the white rat, attempted to study the effects of maze experiences upon subsequent behavior in slightly altered conditions. For this purpose, a maze was designed, which could be altered by the insertion of blinds and the removal of doors, so that the design could be changed by a greater or less amount and in whatever part desired. Five different designs were constructed in this way and were arranged in order, according to their difficulty. This arrangement was not determined by actual experimentation but according to the experimenter's judgment. Eleven rats, 5 normal and 6 blind, were used as subjects. They were trained on maze I and then successively on II, III, IV, and V. The actual order of learning proved to be V, IV, III, I, II, or almost the exact reversal of the suggested complexity. The authors conclude "that in mazes, it is evident that previous experiences are effective upon subsequent behavior and that these effects are advantageous or disadvantageous according to circumstances." The rats when first put into the altered maze ran the first unchanged part without error, showing perfect transfer where the elements were identical. When they came to the altered part and found a blind stop at the end of the former true path, they were inhibited from making the chance turn which would have led them on to the now true path by their former training. Here, then, the transfer was negative. When, however, they at last came upon the old familiar path again, they soon picked up the cue and ran the rest of the way without error. This indicates the complexity of the problem and the many factors involved. The weakest point in the experimental procedure was to assume that an objective criterion of complexity as determined by the number of blind alleys, etc., was accurate. It is perhaps quite unfair to equate blind alleys.

Results from the maze will often show persistent errors, whose elimination requires more time than all the others put together. For some reason these come to have particular significance for certain animals or even for a group. It is, of course, impossible to say that the introduction of this blind alley would increase the errors twofold while the introduction of another alley might increase them but slightly, but there is certainly some necessity for the analysis of this point. In order to produce reliable results Bogardus and Henke should have run a control group of rats on each maze and compared the average time required for learning. Wylie criticizes this experiment further because "in attempting to change only one factor at a time, three important changes were made each time, (1) a change in the direction or amount of turning at the critical point, (2) a change in the length of the runway at the critical point, (3) a change in the order of succession of different runways at the critical point."

This criticism was not made until 1919 and before that time three short experiments and one long study were made of this topic. Hunter and Yarbrough and Pearce (1917) were concerned with the interference of auditory-motor habits and visual-motor habits respectively. In both cases white rats were used as subjects. The apparatus was the T-shaped discrimination box and the rats were trained to turn a certain direction if the stimulus was present and the opposite direction if it was absent. When they had learned this, they were then taught the opposite habit. Hunter and Yarbrough report that there was a forward acting interference between the first auditory-motor habit and the second. Pearce reported the same result with visual-motor habits.

Ruger (1918) performed some experiments in the transfer of habits in the white rat. The first experiment was concerned with the effect of previous semi-circular canal practice on maze learning. The results were found to be negative. The problem here presented was somewhat different from the others. In the case of the semi-circular canal practice the animal was put in a car and carried around the correct path of the Hampton Court maze and then the animals were allowed to learn the maze by the usual method. An interesting problem is presented as to whether any animal could take advantage of such a situation and if an increased number of trials of being carried through would increase the amount of learning. The second part of the experiment consists of chang-

ing various conditions, as learning the last half of the maze, when errors were impossible, and then opening the blind alleys and noting the effect; learning the last half first then the whole maze. Certain other changes as rotation, lining the maze with black paper, etc., were also used but in all cases even when it was somewhat disturbed the old habit soon asserted itself. Here the original habit was not changed but merely certain inhibitory factors were inserted so that the problem was not so much transfer as the effects of certain distractions on a well-formed habit.

Webb (1917) studied the transfer effects in maze learning with rats and humans. He used six mazes for the rats and the pencil mazes used with the human subjects were the same in design. Five groups of rats learned maze *A* and then one group learned *B*, one *C*, one *D*, etc. Untrained animals learned each of the mazes which were used after the training on *A* by the above groups and then learned *A*. Transfer effects were measured by all three criteria, time, errors, and trials, and in every case the transfer was positive. In some instances negative transfer was also involved as in the case of mazes *A* and *F* which were so similar in one part that th- learning of *A* interfered with the learning of *F*. Here, howe ever, as stated above, the total result was positive transfer. Similar results were obtained with human subjects. In the second part of the experiment he found a positive correlation between the difficulty of the first maze as measured by the first maze and the degree of transfer. Shepard in his yearly review in 1917 pointed out that the differences of the compared mazes were greater where transfer was from the single maze to the compared maze than when transfer was from the compared mazes to a single common maze. In the third part of the experiment Webb tested the effect of interpolating the learning of a new maze between learning and relearning of another, *e.g.*, learning *B* then *A*, and then *B* again, and similarly for the other mazes and compared the results with the learning and relearning of that maze when there was no interpolation of another maze. There was greater disintegration in the former case than in the latter, and the disturbance caused in in the old habit by learning the new habit was proportionate to the complexity of the new maze.

Wiltbank (1919) continued the work of Webb in order to study the kind and degree of transfer in white rats which were allowed to learn a maze after they had learned, either completely or par-

tially, one or more other mazes. He used 47 white rats on five different mazes in various combinations, *A-B*, *B-C*, *C-D*, *D-E*, *E-A*, *E-D*, and got positive transfer in every case with errors and time, but in two cases there was negative transfer with trials. The next step in the analysis of results was to determine what effect identical parts had in transfer from one maze to another. He found that such transfer was not due to the prevention of errors in the identical part. He made no attempt to determine whether or not the identical part exerted a favorable influence in saving errors elsewhere. After this was determined each group of rats was allowed to continue its learning until it had completed the whole set of mazes. The transfer was found to be persistent throughout the whole series of mazes but it was not cumulative and the degree of transfer was not a function of the serial position of the maze but of the kind of maze. There was a positive correlation between the "savings" in trials and errors, in trials and time, and in errors and time. Further experimentation by Wiltbank showed that transfer effects between two mazes when the first was only partially learned appeared with the time criteria after the second trial but not with the other two until 16 trials were given in the former maze. On the other hand transfer from a completely learned maze to one partially learned was positive after 2, 4, 6, or 8 trials, but was negative after 16 trials. Wiltbank believes that positive transfer is due to: (1) the recession of the instinct of timidity carried over from one maze to the other; (2) the kinesthetic habits acquired while traversing the true path under the influence of the exploring instinct of curiosity; (3) the association formed between the animal's running the maze to the end of the true pathway and its gaining access to the food box; (4) practice in the elimination of errors, *e.g.*, habitual resistance overcoming tendencies to enter blind alleys.

Wylie (1919) approached the problem of transfer more according to the method of Hunter and Yarbrough and of Pearce (described above). He purposed to investigate the problem of generalized response in order to discover whether a response, which has been learned for one stimulus or situation, can carry over in any degree for a stimulus or situation which is different in a definite way from the first stimulus or situation. He considered that in the maze problems the situations which confront the subject are so similar that it is hard to say just when certain stimuli are removed

and others inserted so he used a different situation, as the problem, viz., a discrimination box. One hundred and seventy-six white rats were used as subjects with three stimuli, light, sound and electric shock. In some instances the animal was taught to choose light, which was given in irregular order from right to left. When this habit was established, sound was substituted for the light and the amount of transfer noted. The dominant stimulus might be the choosing of sound, the avoiding of an electric shock, etc., followed by a substitution of one of the other stimuli. In every case, he says, "learning of a response to one situation, having a given element or stimulus as the dominant or controlling factor, is a help in learning the same response stimulus, not present before, as the dominant controlling factor; that even in some situations the learning process for one sort of dominant stimulus is actually reduced in length so much by first introducing another dominant stimulus, that the time and effort for both is less than for the one alone." He asks, "Would it be too much of a hazard to hypothesize thus: Variations in stimuli allow of positive or advantageous transfer effects, while variations in response, an aspect of the problem which has not been tested in these experiments, produce negative effects?"

Dashiell (1920) reported some transfer factors in maze learning by the white rat. Nineteen white rats were divided into three groups, and each group was given different preliminary tests before they were all taught the maze *Q*. Group I was taken from the nest at regular feeding time and placed upon a table, where the food box had been placed. They learned to enter the food box by pushing open the wire mesh swinging door. A similar introduction to food box and door was given to all the other groups and was followed by further preliminary training. Group II was made to pass through the single passage Na before entering the food box. This run was repeated daily for ten trials. Group III had to learn a definite maze problem. Group IV ran 25 different mazes on 25 successive days. This general conclusion is drawn from the learning records of these four groups on maze *Q*: "We seem to have isolated as factors in the transfer of maze learning ability: (1) adaptation to a general maze situation, *i.e.*, presence of cul-de-sac; (2) adaptation to the materials out of which maze is built (a possible small transfer); (3) the habit of eliminating certain constant errors (a possible small transfer); (4) adjustment in the form of a tendency not to repeat an error in a single run; (5) definite orientation to food box direction."

Because of the difference in the methods, apparatus, etc., used by the experimenters who have worked on transfer of training, it is impossible to draw any general conclusions with regard to it. It seems fairly well established that under certain conditions positive transfer can be obtained and under certain other conditions negative transfer is found and that such transfer may be both general and specific in nature according to the conditions of the problem.

Conclusion

An attempt has been made to bring together the methods, experimental results and general conclusions which have important significance to the various problems which have been approached in the field of animal habit formation, in the hope that future experimentation in this field will profit by the results of previous investigation. The chief contributions of the work of this decade seem to be: (1) the development of more objective methods of study; (2) pioneer work with respect to a number of important problems; and (3) certain theoretical implications (with regard to the mind-body problem, laws of learning, etc.) which have affected the entire field of psychology. The work of the next decade should include (1) the establishment of reliable methods and apparatus; (2) repetition of the problems attacked in the past, in order to test the results of previous investigation and (3) further refinement of the various problems, so that better controls can be used and the results which are obtained be made more reliable.

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SPECIAL REVIEWS

W. H. Pyle. *The Psychology of Learning: An Advanced Text in Educational Psychology.* Baltimore: Warwick & York, 1921. Pp. 308.

The author's preface opens as follows: "In this book I have tried to state everything that is known about learning. All the experimental work that throws any light on the nature of learning has been carefully examined, and in the light of the experimental results I have endeavored to give the present impartial verdict of educational psychology. Throughout, I have tried to keep within the facts. When the evidence does not warrant a definite conclusion, I say so. The few cases of theory and speculation are plainly labeled as such. The practical significance to the teacher of the facts discussed is pointed out."

The topics presented in the volume are as follows: the nature of learning, the learning curve, economical learning, ideational learning, the retention of experience, the nature of learning capacity, measuring learning capacity, differences in learning capacity, transference and interference, fatigue and learning, inborn nature and learning, and statistical measures. Graphs and tables presenting experimental data occupy a prominent place in the discussion of the topics. Each chapter concludes with a selected bibliography. The presentation of material is objective and the interpretative position taken is conservative.

The criticisms which I wish to urge are chiefly with reference to characteristics which the present book shares with others. But before passing to these, it seems worth while to state that the volume does not live up to the promise of its preface. It does not state everything that is known about learning and it does, from practical necessity, contain theory and speculation which are not plainly labeled. Whether or not the text is advanced is somewhat a matter of personal opinion; but it does not presuppose a previous course of study of the same topic. No account is given of the data on learning derived from the study of animal behavior, and much of the excellent German work on memory is missing. Such omis-

sions cannot be justified from thestand point of a comprehensive psychology of learning, although they may be from the standpoint of relevancy to educational problems.

This criticism brings me to those foreshadowed above. The problem of learning is a problem in general psychology, to which science belongs likewise the treatment of other phases of human consciousness and behavior. Educational psychology, so far as it now exists, deals with those characteristics of human nature which are peculiar under school conditions. If the field is not so limited, it takes over all of psychology and much of biology, sociology, and philosophy. If it is so limited, educational psychologists are free at once to attack the psychological problems peculiar to the educational system. My second criticism is not only applicable to Pyle's text but goes beyond it to a condition of which the book is the partial product. It is unfortunate for the broad scope and sound development of a science to be under the care of men chiefly interested in its practical applications. Such a situation, although it may bring an accelerated growth at first, will finally impede further progress through lack of the necessary pure science data. I know of no comprehensive book on the psychology of learning written by a psychologist for other psychologists. Only one approximates this status, and that one is also labeled educational psychology.

WALTER S. HUNTER.

THE UNIVERSITY OF KANSAS

I. B. SAXBY. *The Education of Behavior, A psychological study.* London: University of London, 1921. Pp. 248.

This book was written, according to the author, "to bring our present knowledge of psychology to bear on the problems of behavior which have to be faced by those who are in charge of boys or girls during their adolescence." The early chapters treat of inherited nature under the heads *impulses and reflexes*, *some important impulses*, and *sentiments and complexes*. Later chapters are on *notes on the function of the nervous system*, *growth and control of habits*, *emotion and sympathy*, two chapters on *character*, and a chapter on *work and play*.

It is difficult to see the need of a book of this type. Useful books in psychology should contain either reports of original investigations of the author, or technical discussion and criticism of experimental work of others, or a systematic presentation either

of the whole field or of some special field of psychology for the use of students. In the opinion of the reviewer this book falls into none of these classes. There are but few references to original sources. At the end of the book is one page of references to psychological books. James' "Textbook of Psychology," *Macmillan*, is cited. There is no such book.

W. H. PYLE.

UNIVERSITY OF MISSOURI

M. F. MEYER. *Psychology of the Other-One.* Columbia, Mo.: Missouri Book Co., 1921. Pp. 422.

The instructor who examines this book with a view toward using it as a text in psychology needs to bear in mind the author's caution that the book is designed for students who have little knowledge of physics and chemistry and perhaps still less of biology, but who wish to prepare for work in education, law, or the social sciences. The reviewer is at present using the book in a class in elementary psychology largely made up of freshmen.

The point of view presented by the author is strictly "behavioristic," although this term is not used in the book; obviously because of the extremely narrow and futile meaning which the term suggests to many readers. In context the book resembles a text in the natural sciences much more than it resembles the so-called "standard" psychology textbooks, even when, in the preface, the latter claim to lean toward behaviorism. The traditional classification of the subject matter according to the schema "the senses, the intellect, the emotions, and the will" is absent. There is, for example, no chapter on emotions. Nevertheless, the student learns what action may be called "an emotion."

Those trained in the natural sciences, provided they are able to ignore temporarily the free and naive style which is introduced for pedagogical purposes, will be struck by the rigorous scientific continuity of the successive topics. One does not expect, in a scientific text, such statements as, "If we, accepting the responsibilities of the Creator, desire to improve the conduction, we do that by introducing differentiated conductive tissue" (p. 63). However, if such statements stimulate the students to spontaneous activity in designing nervous systems which lead to a better understanding of the problems involved in human behavior, it must be admitted that the sentence has justified itself as a teaching device.

Again, one does not expect a chapter on the relation between psychology and the other sciences to begin with the startling announcement that "Robinson Crusoe has just acquired his man Friday" (p. 4). But if this statement gives the student a concrete picture of what is meant by social interaction, rather than a stereotyped verbal definition of psychology, the interests of good teaching again have been conserved even though hallowed textbook traditions have been disregarded.

The book abounds in many innovations of this sort, some of which are apt to obscure its real merits for the uncritical reader, and may even build up prejudices against principles which would have been unqualifiedly endorsed had the scrutiny been carried further. The author presents, in the simplest language, what he regards as the fundamental principles underlying human behavior. The subjective terminology of traditional psychology is avoided, but the chemical, physical, and biological conceptions that are substituted are regarded as entirely adequate to account for the most complex forms of human social interaction.

The first half of the book is devoted to a development of eight fundamental forms of behavior: "(1) Locomotion in a straight line in response to lack of food; (2) turning the body axis sidewise in response to an obstacle; (3) positive localization; (4) negative localization; (5) grasping; (6) adjustment of the sense organs; (7) signaling; (8) sleeping" (p. 210). As contributing to the development of the principles involved in these eight forms of behavior, the author introduces such topics as: locomotion, stimulus and excitation, sensitivity, conductivity, contractility, neurons and the nervous system, concerted action, preoccupation.

In the last half of the book the eight forms of behavior are elaborated into such phenomena as instincts, emotions, perception in all the sense fields, an unusually full and complete analysis of the language reaction, imitation, rhythm, abstraction, generalization, memory, disturbances of personality, abnormalities, and a concluding chapter on "Mysterious Problems" in which the writer tries to remove some of the popular misconceptions regarding the nature of psychological problems.

Anatomical structures and physiological processes are used only to such extent as is obviously and directly necessary for a clear understanding of the principles of behavior. The emphasis is shifted from anatomy to the *function* of the nervous system. The

criticism that is often made by biologists, that psychology is a mixture of out-of-date physiology and worn-out philosophy, certainly cannot apply to Professor Meyer's book. The systematic position of the author is best indicated by his definition of Psychology: "If we call psychology a Natural Science it is the study merely of the nature of 'the Other-one in relation to us.' And if we call psychology a Social Science, it is the fundamental social science" (p. 11). If Dr. Meyer's text actually meets the conditions which he imposes by his own definitions, the contents of his book should offer an excellent background for courses in educational, applied, and abnormal psychology; the social sciences, history, law, philosophy; and the sciences based on medicine.

As a guide in studying, the author has framed about fifteen questions for each chapter. Every question has its answer in the text and in the chapter to which it refers, but the answer is not obvious and must be "dug out." The student who is able to answer the questions readily "knows his text"; mere ability to memorize is placed at a discount. From the standpoint of the instructor these questions simplify quizzes, make it possible to give very specific assignments, and to test the extent to which the material is understood.

For classes that have a laboratory period Dr. Meyer has prepared a laboratory manual of simple experiments that have been carefully integrated with the principles brought out in the textbook. The value of this from a pedagogical standpoint is so obvious that it needs but to be mentioned. Instructors who are well prepared in the principles of the natural sciences will find the book very easy to teach from the beginning. For those instructors poorly prepared in science the text will present many difficulties at first. The brighter science students are very likely to run ahead of the instructor.

Many of the behavioristic interpretations of human behavior found in current psychological literature are too specialized in their application and break down when an attempt is made to state them in the form of fundamental laws. Professor Meyer has recognized this fact more clearly than most behaviorists. In this book he has presented this more fundamental aspect in a simpler and pedagogically more effective manner than in his more technical *Fundamental Laws of Human Behavior*. Both books taken together present the most consistent and thoroughgoing exposition

of the theoretical assumptions underlying behavioristic psychology with which the reviewer is familiar.

Whatever the systematic interest of the reader may be, he should not expect an adequate understanding of the book from a superficial examination of isolated paragraphs or chapter headings. There are many books that can be satisfactorily evaluated by a cursory reading, but Max F. Meyer's *Psychology of the Other-One* does not belong to this class.

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NOTES AND NEWS

Dr. Max Schoen will give a series of seven lectures on the Psychology of Music during the first semester at the Carnegie Institute of Technology.

The Iowa Child Welfare Research Station at the State University of Iowa has organized a Laboratory in Child Psychology for experimental work with children from 2 to 4 years of age.

A new periodical for psychological literature has just appeared under the title of *Psychologische Forschung*, edited by Professors K. Koffka, W. Köhler, M. Wertheimer, K. Goldstein and H. Gruhle and published by Springer, Berlin. The numbers will be published at irregular intervals. The price per volume is M. 86.

Dr. John Dewey, professor of philosophy in Columbia University, has returned to New York after having spent three years in the Orient as educational adviser to the Chinese government.

Dr. H. W. Crane, assistant professor of psychology at Ohio State University, has been called as associate professor in psychology to the University of North Carolina. Besides his university duties, he will act as psychiatrist to the State Board of Public Welfare.

The death is announced, on September 26th, of Professor Alfred Lehmann of Kopenhagen.